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PERSONAL EQUATION.

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The terms "personal equation" and "personal difference" are somewhat loosely used by astronomers to indicate such systematic errors in observation as originate in the observer, in distinction from those that arise from instrumental and atmospheric conditions. But the errors thus grouped together in their place of origin have by no means the same causes. Some are purely anatomical, such as the constant and clear difference which has been found between observers in setting the cross-wires of a microscope on the division mark of a scale,[1] or in bringing a star midway between two parallel wires, the cause of which seems to be asymmetry of the halves of the eye. To the same general class would belong astigmatism and other structural defects of the eye as far as they inter-

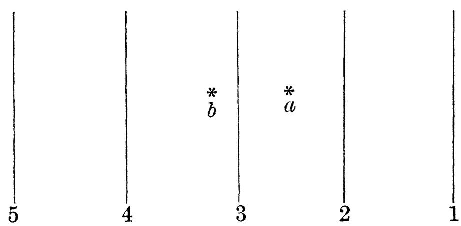
[1] For example, on the limb of a transit circle, or in microscopically comparing standards of length.

fere with observation, and color blindness (if that be an anatomical defect), which has been suggested as explaining the different magnitudes assigned by different observers to the same celestial object. Another set are in part from psychic causes. Such are those that beset observations where judgments of time or space must be made. And others are purely psychic, without physical admixture, like the bias for or against special tenths of a second shown in the recorded observations of some astronomers and recognized more or less consciously by others in themselves. It is, however, a portion of those of the second class that were first noticed, first received the name of personal equation, have since received the most careful investigation, and yet remain the most important. Of the discovery and investigation of these it is the purpose of this paper to give an account.

Every observatory has for one of its chief businesses the fixing of the instant in which heavenly bodies cross its meridian. On this depends the keeping of the true time, and, in connection with the measurement of the distance of these bodies north or south of the equator, the fixing of their positions and motions in the heavens. And in this very process the personal equation is involved. The instrument used for these observations is, in its lowest terms, a telescope mounted on an east and west axis and turning in the plane of the meridian. In the focus of its eye-piece is a set of fine parallel wires or spider-lines from five to twenty-five in number, called a reticle. The middle one of these lies in the meridian. As the image of the star moves across the field, the instant of its bisection by each of these wires is taken, and the average of the times, provided the intervals between the wires are

equal, gives the time of the bisection by the central wire with much less liability to accidental error than if that had been used alone.

At the time of the first notice of personal equation, the method of fixing the instant when the star crossed a wire of the reticle was that of Bradley, or, as it is called, the "eye and ear" method. When the star is about to make its transit, the observer reads off the time from his clock, and then while he watches the star in the telescope, continues to count the second beats. He fixes firmly in mind (as the moving image approaches the wire) its place at the last beat before it crosses the wire and its place at the first beat after, and from the distances of these two points from the wire, estimates by eye the time of the crossing in tenths of a second. A glance at the figure will make the *modus operandi* clear.



The star in most telescopes appears to move from right to left. If we suppose it to be at *a* when the eighth second is counted, and at *b* when the ninth is counted, the time of crossing the third wire will be so many hours, so many minutes, 8.7 seconds. The rôle of the mind in observations by this method is the fixing of the exact place of the star at the first beat, the holding of the same in memory, the fixing of the place at the second beat, the comparison of the two, and the expression of their relation in tenths. When instan-

taneous occurrences like heliotrope or powder signals or the occultation or emergences of stars are to be observed, several ways are open, but the most common ones require the estimation of the fractional part of the second directly by ear. A few astronomers also were accustomed to observe transits in the same way, treating the passage of the wire like an occultation. But this was generally regarded as a vicious aberration from the true method. The psychic action here is a comparison of the two very short intervals of time between the event and the preceding and following clock-beats; or, regarding the whole series of beats, the interpolation of the sudden sensation into their recurring series.

The "eye and ear" method remained the accepted one till about 1850, and is even now more or less used, especially for slow-moving stars like the pole-star. About 1850 the chronographic method of observation was introduced. The chronograph consists essentially in an evenly revolving drum, with which a writing apparatus, under control of an electro-magnet, is connected in such a way that as the drum revolves the apparatus moves slowly from one end of it to the other. If it were undisturbed the pen would trace a spiral line upon the paper with which the drum is covered. But a clock is brought into the circuit with the electro-magnet, and at each second-beat sends a current through it; the magnet draws back the pen and puts a jag in the line for every second except the sixtieth, which is omitted to indicate the minute. A key in the hands of the observer enables him to record his observation by a jag in the same line or a parallel one. All that remains to do then is to indicate the time on the clock to which a certain one of the second-jags corresponds,

and there is a permanent record from which the time of the observation can be read off with ease to a small fraction of a second. By this method of recording the process of observation is much simplified. The astronomer now watches till he sees the star bisected by the wire, then taps his key. He has simply to perceive an event and to will a movement of his finger. The part which the mind plays is thus nearly the same in the observation of transits and sudden phenomena. There is, however, here also a variant application of the method little to be commended. Some observers aim to tap the key so that they shall hear the click of it at the instant of the bisection. They thus add an element of judgment to simple perception and the willing of movement; for to accomplish what they intend, the impulse of will must be given before the star is really behind the wire, and the length of time by which the impulse must precede must vary with the apparent rate of the star. For sudden occurrences they are obliged, of course, to observe like other people.

Now, in all the methods of observation which have been mentioned, observers habitually vary both from the true time and from each other. Their variations from the true time are called their *absolute personal equations;* their mutual differences are their *relative personal equations*. It is natural that the latter should have been first discovered.

So much of a preface has seemed necessary to show what personal equation is. In what follows I propose first to give a brief historical account of the discovery and chief general studies on personal equation, then a more detailed presentation of the circumstances which produce variation in its amount, and, lastly, something of the theories which have been put forward in explanation of it.

The Discovery of Personal Equation.

The first record of a persistent personal difference between the observations of experienced astronomers goes back a little less than a hundred years. About 1795, Maskelyne, the British Astronomer Royal, noticed such a difference between those of himself and his assistant. At the end of the third volume of the Greenwich Observations he writes as follows:

"I think it necessary to mention that my assistant, Mr. David Kinnebrook, who had observed the transits of stars and planets very well in agreement with me all the year 1794, and for a great part of the present year, began from the beginning of August last to set them down half a second of time later than he should do according to my observations; and, in January of the succeeding year, 1796, he increased his error to eight tenths of a second. As he had unfortunately continued a considerable time in this error before I noticed it, and did not seem to me likely ever to get over it and return to a right method of observing, therefore, though with reluctance, as he was a diligent and useful assistant to me in other respects, I parted with him.

"The error was discovered from the daily rate of the clock deduced from a star observed on one of two days by him and on the other by myself, coming out different to what it did from another star observed both days by the same person, either him or myself . . .

"I cannot persuade myself that my late assistant continued in the use of this excellent method (Bradley's) of observing, but rather suppose he fell into some irregular and confused method of his own, as I do not see how he could have otherwise committed such gross errors."

To the unastronomical mind a difference of eight tenths of a second seems small, but its real signification is more apparent when it is multiplied by fifteen, to give seconds of arc.

For the next twenty years this germ of a discovery lay dormant. But in 1816, von Lindenau mentioned the incident in a history of the Observatory of Greenwich in the *Zeitschrift für Astronomie*, and there it fell under the eye of Bessel, the celebrated Königsberg astronomer. Later, the English Board of Longitude sent the latter a copy of Maskelyne's observations, from which he got a more complete knowledge of the facts. The case impressed him. Considering the easy conditions of such observations with good instruments, and that such were regarded as sure to one tenth or at most two tenths of a second, a difference of eight tenths seemed wellnigh incredible. Its continuance, too, in spite of the desire that Kinnebrook must have felt to bring his observations into harmony with those of his superior, went to prove it involuntary, and therefore important alike to astronomy and anthropology. Bessel desired to know whether such a difference could be found between other pairs of astronomers, and in 1819, while on a visit to Encke and von Lindenau at the Observatory of Seeberg near Gotha, he proposed to test the point with them. Each observed the culmination of several stars, but no second clear night during his stay allowed them to complete the comparison, and the question remained unanswered.

In the winter of 1820–1, at Königsberg, he returned to the subject and made comparisons with Dr. Walbeck, by transits observed on the meridian circle of the observatory. They observed ten stars near the equator

on several nights, each observing five a night, and alternating in such a way that those observed by Walbeck on one night were observed by Bessel the next, and *vice versa*. In this way they arrived at determinations of the rate of the clock which should differ by double the amount of the personal difference, and were thus well calculated to show it if any existed.[1] They found that Bessel was always in advance :

		s.
Dec. 16 and 17	. . . . . .	1.145
" 17 " 19	. . . . . .	0.985
" 19 " 20	. . . . . .	1.010
" 20 " 22	. . . . . .	1.025
	In the mean,	1.041

This result Bessel considers exact within a few hundredths of a second. The difference was striking on the second day and led naturally to redoubled efforts for accuracy. "We ended the observations," says the astronomer, "with the conviction that it would be impossible for either to observe differently, even by only a single tenth of a second."

Later he repeated the experiment with Argelander, using a little different method. In 1821 he observed seven stars in Gemini, each six times, under favorable circumstances, and their mean position for 1820 was calculated. On two evenings in March and April, 1823, Argelander observed the same stars, while Bessel himself determined the clock corrections. The result-

[1] Suppose the clock to be gaining and that Bessel observes earlier than Walbeck. Then for the stars which Bessel observes first and Walbeck second, the clock rate found will be the real gain of the clock plus the difference of the observers. When Walbeck observes first and Bessel second, the rate found will be the gain of the clock minus the difference of the observers. The difference to the two rates of gain found will be double the personal difference. If the clock is losing, the case is similar.

ing right ascensions were in excess of those previously found by Bessel, and in excess of what they would have been if Argelander had observed the clock stars himself; that is, the stars appeared to Argelander to cross the meridian later than they did to Bessel. The mean difference for the day in March was 1.222 s., for the day in April 1.224, whence B — A = — 1.223 s.[1]

Bessel, however, was not content here; Walbeck and Argelander were less practiced in transit observations than he, and he thought that possibly the cause of the difference lay in this. He accordingly asked Struve, of Dorpat, to compare observations with him by means of comparisons with Walbeck and Argelander as they passed through his city. In 1821 Walbeck and Struve observed together on four days, with the resulting equation:

$$S - W = -0.242^{s.}$$

whence

$$B - S = -0.799$$

In July, 1823, Argelander obtained the following:

$$S - A = -0.202^{s.}$$

whence

$$B - S = -1.021$$

The personal difference, therefore, did not originate in difference in practice.

There is, however, a difference of 0.222 s. between the two values for B — S, and, since there is very little uncertainty in the individual determinations, is evidence of change in one or another of the four observers; most probably in B or S, for the comparison of the intermediary with S was made each time soon after that with B. A single direct comparison points the

[1] The statement of the personal difference in this form has led to its being called the "personal *equation*."

same way. In October, 1814, Struve visited Bessel, and the two observed together ; Struve observing the transit of one star, Bessel of two. From these by calculation the equation B — S = — 0.044 s. is found, and though it rests on a single transit, is not without weight, for Struve considered the observation successful, and the agreement of the single wires testifies the same. At any rate the error was not one of eight tenths of a second.

This is sufficient to establish the variability of the personal equation ; but later comparisons (leaving Bessel's first study for the moment) give further evidence of the same thing. In 1825 the visit at Königsberg of another astronomer, Knorre, who had just compared with Struve, gave opportunity for repeating the determination of B — S. The result was, B — S = — 0.891 s. A direct comparison in 1834 gave B — S = — 0.77 s. Taking all together we have :

		s.	
1814,	B — S	= — 0.044,	direct comparison.
1821		= — 0.799,	indirect "
1823		= — 1.021	" "
1825		= — 0.891	" "
1834		= — 0.770	direct "

Bessel's next thought after having established the fact of a personal difference, was to find its cause. To that end he began to vary the conditions. He first substituted the sudden disappearance or reappearance of a star, as in occultations and emergences, for its steady motion across the reticle. Seventy-eight comparisons of this kind gave for Bessel and Argelander, B′ — A′ = — 0.222 s.; another set of twenty-one gave B′ — A′ = — 0.289 s. A comparison of Struve and Argelander on these sudden phenomena developed no

significant personal difference. Observations of this kind are less certain than transit observations, but they seemed to Bessel to indicate that the trouble lay in combining the steady advance of the star with the sudden beat of the clock, and his next experiment was therefore with a variation in the clock. On two nights he observed a chosen series of stars with a clock beating half seconds, with the following result (indicating by B″ his observations with the half-second clock):

$$\text{On the first night } B - B'' = -0^{s}.520$$
$$\text{" " second " } B - B'' = -0.467$$

That is, he observed transits later, on the average, by about half a second in this way than with the whole-second clock. Argelander's observations on the half-second clock compared with those of Bessel made in the ordinary way showed no particular change: $B - A'' = -1.246$ s., or, in another series, $B - A'' = -1.208$ s.

Observations with a half-second clock at Dorpat gave $S'' - A'' = -0.227$; from all of which it appears that Bessel alone had his personal equation changed by the alteration of the rapidity of the beat. One other point the Königsberg astronomer investigated, namely, the effect of the apparent rate of the star, which varies with its declination, on the personal equation. This is of great importance, for if it be found that the rate has no effect, then, provided the personal equation is constant for the time being, it will affect equally the times of transit of all stars observed by the same observer, and will not change at all their relative times of transit, on which their right ascensions depend. Bessel varied the apparent rate of

motion by the use of different powers in his eye-piece, and concluded that the rate had no influence, at least for differences equal to those from the equator to within 30° of the pole.

In brief, Bessel established these points: the fact of personal equation, its spontaneous variation in considerable periods of time, and its artificial change, for himself at least, with change of the clock beat and from transits to sudden phenomena, and he tried, with negative results, the influence of the rate of motion. How important these discoveries are in relation to present knowledge will appear as the narrative proceeds.

Bessel's theory of the psychical cause of the personal equation which he had discovered will be considered elsewhere. In brief it is that the work of the mind is the comparison or superposing of the unlike impressions on the eye and ear, and that observers differ in the readiness with which they accomplish this; an additional difference coming in when one of them goes over from seeing to hearing and the other from hearing to seeing.

It has more than once been noticed as a fortunate coincidence for the knowledge of this matter, that the discoverer of the personal equation should himself have had so large a one; and such it probably was. But its very size has provoked incredulity. It seems simply impossible that two practiced astronomers should observe the transit of a star differently with a clock beating seconds by almost a beat and a quarter. Encke has contended, and after him Wolf, that Bessel must have differed from other observers in the counting of his seconds; counting, for example, a transit which occurred seven tenths of a second after the

pointer of the clock had passed the fourteenth division of the face, as 13.7 seconds instead of 14.7 with other astronomers; in other words, adding the fractional part to the second completed instead of the second begun.[1] The strongest argument in support of this view is that furnished by Bessel's own experiment with the half-second clock, where the average of the two equations obtained gives $B - B'' = -0.49$ s., showing that he observed later by a half second when he timed the transit by half seconds—exactly what would occur if it was his counting that was at fault. And Wolf cites as further evidence the case of a Parisian observer whose results differed from those of his colleagues by a whole second, until the matter was forcibly brought to his attention by setting him to observe the disappearance of a moving object behind an obstacle while he counted the seconds out loud and some one else marked the instant of phenomenon for him by a blow on the back. And there have been other instances of the same kind.

On the other hand, Peters shows that the personal equation between Bessel and Argelander is not unique, as it should be if the former, as Encke says, had counted his seconds "too early as against all other astronomers," but rather the last term of a series of which lower terms can easily be shown. For example:

			s.
Oct. 7,	1833.	Nehus—Wolfers	=0.62
" 8,		" "	=0.84
	1837.	Gerling—Nicolai	=0.78
	1854.	Main—Rogerson	=0.70

[1] Such a possibility is also explained as follows: The audible beat of the second is made when the pointer is still moving from mark to mark on the dial. A difference of a second would be introduced if one observer associated the beat with the dial mark which is left and the other with the one that is approached.

To these may be added, not counting Maskelyne and Kinnebrook,

	1843.	Goujon—Mauvais = 0.58
and	1859.	Sashoo—Jacobs = 0.80

Again, if the size of Bessel's personal equation had been due to his method of counting the seconds, his observations of sudden phenomena should have shown it as well as his transit observations; whereas the former gives $A' - B' = 0.222$ s., and the latter $A - B = 1.223$ s. That Bessel's method of counting was the same in the two cases, Peters testifies from conversation on the subject with Bessel himself and from observations made by Bessel in his presence. The presumption is also natural that the possibility of a difference in counting must have suggested itself to a mind so fertile as Bessel's. Wolf is right in judging the fact a hard one to explain on either hypothesis, but it seems to me rather less hard on the supposition that the astronomer counted his seconds correctly than on the other.[1]

PERSONAL EQUATION BEFORE THE INVENTION OF THE CHRONOGRAPH.

In the years following Bessel's discovery there were occasional recognitions of personal equation. That of Wolfers and Nehus determined in 1833 has already been given. At an early date, also, Dr. Robinson, of the Observatory of Armagh, noticed a difference of personal equation as the first or second limb of the sun or moon was observed; but not till about 1838 does it

[1] The fact that Bessel observed with Encke and von Lindenau in 1819 without finding any personal equation has been cited as evidence that he counted his seconds correctly. But as has been said, no personal equation was found, not because there may not have been one, but because the comparisons that were to show it could not be completed.

seem to have received much consideration in actual practice. In the volume of the Greenwich Observations for that year, Airy, the Astronomer Royal, began to publish the figures for the personal equations of the transit observers under his charge. During the two years previous, differences had been noticed, but were too small to be significant. The figures were not found by special tests, but calculated from the clock errors observed in the routine work of the observatory. In the same year, Gerling, professor in the University of Marburg and director of its observatory, published the result of the measurement of the longitude of Göttingen, Marburg and Mannheim, made in connection with Nicolai, Gauss and others. After the measurements had been completed the observers compared among themselves for personal equation, and made the longitude observations uniform by reducing them all to Gerling's own as a norm. The following are the figures obtained by these comparisons:

For transits:

	s.		
Gerling—Goldschmidt	=0.195	from 26	obs. of stars.
Gerling—Nicolai	=0.783	" 72	"
" "	=0.681	from 190 observations of the transits of a spring pendulum.	
Gerling—Hartmann	=0.051	from 180 observations of the transits of a spring pendulum observed with a half-second clock.	

For flashes of light:

	s.		
Gauss—Goldschmidt	=0.088	from 292	observations.
Gerling—Goldschmidt	=0.027	" 56	"
Gerling—Nicolai	=0.157	" 308	"
Gerling—Hartmann	=0.055	" 267	"

As early as 1842 it occurred to Arago that personal equation might be reduced or abolished by giving the observer but one thing to attend to. On New-year's-day, 1843, he applied his idea in the case of a young astronomer, Goujon, whose personal equation usually reached about half a second. He had Goujon indicate the passage of the star by a quick stroke, while another observer, Bouvard, kept the time and estimated the fraction of a second. The personal difference disappeared. To remove the doubt that the difference might have been due to a slowness of hearing, he caused a third person to give the taps while Goujon and Bouvard took the time together. Again they agreed through the forty trials made. During the same year Arago made further tests with a *chronomètre à pointage* which was so constructed that on the pulling of a trigger the second-hand made a dot on the dial from which the fraction of the second could be read off. The observer had only to pull trigger at the instant of the transit and his record was made. With this instrument, Goujon and Mauvais, who otherwise differed by 0.58 s., observed alike. The limit of accuracy in these comparisons was about one twentieth of a second.

In 1843 and 1844, Otto Struve measured a number of personal equations in connection with the determination of the difference of longitude of Pulkowa and Altona, but the figures are of no particular consequence to the subject in hand.

The Invention and Advantages of the Chronograph.

The first attempt at the simplification of transit observations by reducing them to the indication of the instant of passage, and the first suggestion of the chro-

nographic method, antedates the experiments of Arago just recounted by fourteen years. In 1828, J. G. Repsold, director of the Observatory of Hamburg and celebrated mechanician, proposed an apparatus in which the record was taken by means of a point connected with a key on a strip of paper regularly moved by clock-work. In taking a transit observation, the machine being in motion, the observer was first to record a beat of the clock by a tap of the key just before the star began to cross the reticle, then in the same way the crossing of each spider-line as it occurred, and finally the stroke of the clock following the crossing of the last one. From the distances of these dots the time was to be measured off. It was essential, of course, that the strip should move evenly from the first dot to the last, about three minutes for equatorial stars. But the apparatus first made being without a governor, failed in this particular, and the death of the inventor prevented the perfecting of the instrument.

The chronographic method, as it is now practised, is an American product—so distinctly so that it is frequently called "the American method." Of its origin Professor B. Pierce speaks as follows: "The American method is the unquestionable product of the Coast Survey of the United States, and was the legitimate result of the rigid and profound methods of research which are uniformly adopted in this magnificent work. The first conception was in the mind of the superintendent himself, Professor Bache, and its complete development and ultimate success were owing to the united action of Professor Bache and his friend and assistant, Mr. Sears C. Walker. The details of the instrumental invention and execution were intrusted

to Messrs. Saxton, Bond, Mitchel and Locke. Different plans were proposed, but that of Mr. Bond is the one which is at present [1860] adopted in the Coast Survey."

The principle of the chronograph has already been described, and it would be aside from the subject in hand to notice the variations in detail which have been introduced. Suffice it to say here that those in most common use to-day show variations in detail only.[1]

In 1851 the Bond chronograph was exhibited at the meeting of the British Association, and in 1854 the method was introduced at Greenwich. From time to time other observatories have followed and the method is now the accepted one.

The adoption of the chronograph did not do away with personal equation, but it greatly reduced it. Out of thirty-four personal equations determined at Greenwich from 1854 to 1856, only four exceeded 0.1 s., and the highest was 0.17 s.; but in the three years previous, by the old method, out of thirty-three, nineteen exceeded 0.1 s. and eight were over 0.17 s. The difference is due in part to a change of observers, but is nevertheless significant. From an astronomical point of view, however, the increased certainty of the observations is of far greater importance than the lessening of the amount of personal equation. Dunkin found from a comparative study of the observations made on the Greenwich transit instrument in the last year of the "eye and ear" method (1853), with those on the same instrument in 1857, that the probable error of an observation at a single wire by the "eye and ear" method was ± 0.074 s., the probable error of a

[1] Chronographs have been devised which should give the instant of an observation in printed figures, but they have not, I believe, yet reached perfect action.

complete transit ±0.028 s.; the probable error of an observation at a single wire by the chronographic method was ±0.051 s., that of a complete transit ±0.017 s. Approaching the same question again in 1864 from another point of view, he arrives at figures for the probable error of an observation at one wire by the two methods which show the effect of the change on individual observers :

	Henry.	Dunkin.	Ellis.	Various observers, mostly less practised.
Eye and ear,	± 0.112 s.	± 0.062 s.	± 0.069 s.	± 0.089 s.
Chronograph,	± 0.058 s.	± 0.048 s.	± 0.053 s.	± 0.060 s.

Other advantages are credited by Dunkin and others to the chronographic method, but among these the point of special interest in this connection is that the personal equation seems less variable in its amount.

Ways of Determining the Amount of Personal Equation and Devices for Excluding it in Observation.

The reduced personal equation that persisted in spite of the chronograph was still, in the eyes of astronomers, a blemish on the fine accuracy of their science, and from time to time efforts were made for some means, either of determining its amount exactly so that it could be taken into calculation, or of changing the method of observation so as to exclude it. Three ways of determining the amount of personal equation have already been mentioned, that of the Greenwich Observatory, where the custom long has been to get it from the clock corrections found in the routine work of the observatory, and the two ways used by Bessel in comparing with Walbeck and Argelander. Another is the method of divided transits: both observers use the

same instrument and observe the same culmination, one observing the passage over the first wires of the reticle, the other over the last, changing the order in which they observe from star to star so as to exclude possible errors in the corrections for the distances of the wires. This was the method in most common use where personal equation was found from special comparisons. Its chief advantage is that all the instrumental conditions are the same for both observers; its chief disadvantage, provided both are equally accustomed to the particular instrument used, is the hurry of changing places, which might prevent the second observer from observing as he would at his leisure. A special "binocular eye-piece" that was designed to avoid this difficulty was tested at Greenwich in 1852 and 1853. An equilateral prism set in the eye-piece gave two views of the transit from positions 120° apart, thus enabling observers to compare without inconveniencing each other. A method of determining personal equations with transits of the limbs of the sun also allowed simultaneous comparison of a number of observers. The images of the sun and the reticle were projected from the telescope on a table or semi-transparent screen, and the transits were observed as they occurred there. There are still other methods, *e. g.* the observation of the same phenomena with adjacent instruments, or the determination of the longitude of points whose distance is already directly known. But there remains one that deserves attention, namely, that of artificial transits. Its advantage is that the phenomenon to be observed can be produced at any time and as often as necessary. Gerling seems to have been the first to get at the personal equation in this way. When he was comparing him-

self with Goldschmidt and Nicolai, at the suggestion of Gauss, he made use of the transits of an inverted spring pendulum (*Kater'scher Feder-pendel*) in addition to transits of the stars. At an early date Prazmowski used the vibrations of a declination needle for a similar purpose. Such methods are applicable, provided that the personal equation remains the same for the artificial transits as it is for the real, a condition which is probably much better fulfilled in some apparatuses than in others. Against all comparisons by means of real stars, on the other hand, it may be urged that the atmospheric conditions which make a star at one time clear-cut and at another time "woolly," interfere also with the accuracy of the results.

As intermediate between the way of getting rid of personal difference by fixing its amount and allowing for it, and those of excluding it in the observation, two practical devices may be mentioned for avoiding it without knowing its amount. Error is not brought in unless the observations of astronomers between whom such a difference exists are united in computation. This is guarded against by indicating with each observation by whom it was taken. In observations for longitude, however, the combination of the work of two observers is a matter of necessity, and here it is customary for them to exchange stations. Both plans, it will be seen, assume that the personal equation remains practically constant, a thing that seems to be sometimes true and sometimes not.

The devices for excluding personal equation at the moment of observation aim to carry the simplification beyond the point reached by the chronograph. That left the observer free to concentrate his attention on the star; these do away with the motion of the star;

and one even goes so far as to do away with the observer himself. The way in which the first is accomplished is by giving to the whole instrument,[1] or to the reticle,[2] a motion equal to that of the image of the star. This allows the observer, since the motion is under his control, to bisect the image with a line of the reticle as exactly as if both were at rest. When the bisection has been accurately made, the position of the instrument at a certain instant and the time are recorded by the observer or automatically, and from the record the time of the transit of the meridian is calculated. Another means to the same end is instantaneous illumination of the wires.[3] The illuminating flash is made to occur at intervals exactly equal to the time required by the image of the star to move from wire to wire, and its occurrence is recorded, together with the beats of the clock, on the chronograph. The beginning of the series of flashes is under the control of the observer, and is made by him to coincide exactly with a bisection, three or four trials generally being needed; after this the flash repeats itself and its record on the chronograph at each bisection. The rate at which the flashes recur is also adjustable to the declination of the star. The instantaneousness of the flash makes the image to all intents stationary at the instant of bisection.

The difficulty with these methods is the complex apparatus which they require. An instrument of this kind, to be of any service, must be adjustable through a considerable range to the apparent rate of the stars.

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[1] Liais.

[2] Radier and C. Braun. Suggestions for something of the same nature were made by Wheatstone, and, I believe, by A. S. Herschel.

[3] Langley.

The proposal to exclude the observer himself comes from M. Faye. He suggested the substitution of a sensitive plate for the eye of the observer, and the instantaneous photographing of the wires and the image, the instant of the exposure of the plate being recorded electrically. This answers best for transits of the sun where there is plenty of light, but is not impossible for stars. Professor Langley thinks "it is perhaps not too much to say that it will probably be the method of the future."

INVESTIGATIONS OF THE ABSOLUTE PERSONAL EQUATION.

So far the relative personal equation alone has been spoken of. As long as this alone had been measured, astronomers could be told that though they knew how much they differed among themselves, not one of them knew how much he differed from the truth. They were therefore naturally curious to know what their differences from this were—in other words, what their absolute personal equations were. And the question had besides interesting ramifications into physiology, psychology, and anthropology. Artificial transits and electrical appliances for recording already in use gave the means required for these measurements, and they were soon begun. In 1854, Prazmowski suggested an apparatus for this purpose. It was to consist of a disk carrying a luminous point for a star, and closing an electric circuit the instant the image of the star was bisected by a line of the telescope through which the transit was observed. The second, the instant of observation, and the true time of the transit were to be recorded by electrical means on a moving strip of paper. By varying the distance of the telescope and

the rate of the disk, the conditions of actual observation as regards power of the instrument, rate of the star, etc., could be paralleled. The apparatus could be used for observations by "eye and ear," by taking the seconds from the click of the electro-magnet that recorded the seconds on the strip.

In 1856, Professor Mitchel announced an apparatus for the measurement of absolute personal equation. Two years later he communicated to the English Astronomer Royal the result of a series of experiments on the subject. He used ten artificial stars attached to a revolving disk, recording their real transits electrically, while the observer did the same in a similar way for their observed transits. These records corrected for the errors of the apparatus give the absolute personal equation, or, as Mitchel calls it, the "absolute personality of the eye." The "personality of the eye" he measured both for transits and for the perception of a white stripe on a dark ground; the "personality of the ear" and "personality of touch" likewise by stimuli suited to those organs. He and his assistant, Twitchell, made daily observations for sixty or seventy days, and about thirty persons besides themselves were tested. The following figures are the means of two hundred and fifty-five observations each, the eye stimulus being the white stripe and the ear stimulus a quick tap:

		Mean. s.	Minimum. s.	Maximum. s.
M.	Eye,	0.161	0.139	0.191
	Ear,	0.164	0.143	0.193
T.	Eye,	0.144	0.118	0.184
	Ear,	0.153	0.129	0.201

Special tests were made to find whether the eye and ear were constant in their "personality" for short

periods of time. Mitchel and his assistant on several days took sets of ten observations each in alternate minutes, and found the eye personality liable to variations of as much as 0.020 s. between the sets of ten. Touch gave results similar to those for the eye, and experiments were not continued.

In taking the artificial transits, Mitchel found that he himself, his assistant, and all the persons tested anticipated the true time. For himself this anticipation was on several occasions as great as 0.1 s. on a mean of ten, and showed somewhat of a daily variation. This led to the trial of artificial emergences and immergences. The first gave results like the simple observation of the white stripe; the second showed the tendency to anticipation and less steadiness. To put what he had discovered to practical use, he replaced the spider-lines in the reticle of his instrument, except the central one, with occultating bars, and observed by immergences, emergences and transits of the central line, but the effect of the change he was not able at the time to report.

In the same year, 1858, Julius Hartmann, Professor in the Lyceum at Rinteln, also published the description of an apparatus for the same purpose, and the results of a study made by means of it. His apparatus consisted of a horizontal clock controlled by a conical pendulum, the regularity of which was tested by a siren. A wheel carrying a three-inch disk of paper was made so as to shunt in or out of the clock system, and could be set so as to produce at any fixed hundredth of a second a sudden flash through a little hole in the disk, or the transit behind a white thread of a steel bead on the surface of the disk. These were to be observed by the "eye and ear" method, the clock

itself giving the second-beats. The conditions could be varied by changing the distance of the bead from the centre of the disk and by changing the distance of the observer. The maximum error of the machine was not more than 0.03 s. or 0.04 s.

In using his apparatus, Hartmann was accustomed, when once the disk was set at any fraction of a second, to let the phenomenon to be observed recur again and again at periods of eight seconds (for light flashes at first even every second) till it could be observed, as it were, at leisure. The result was often a considerable difference in the answers made at the beginning and end of the process. In this way, in his opinion, the observer quickly got an observation free of surprise.

The interest of these repetition experiments is perhaps other than the experimenter realized. Not only is surprise avoided, as he supposed, but the nature of the psychic process is changed. An observer soon catches the rhythm of such a recurring series, and as each member of the series comes, it finds the mind in a state of active expectation. As experimenters in the psychological field have since shown, the reaction time for an expected stimulus is very much abbreviated—so much so, indeed, that the reaction may even precede the stimulus which it should follow. Perception does not then lag behind sensation; the inner or mental series is pushed forward in expectation and synchronized with the outer actual series of stimuli.

The figures for the personal equation found by these experiments are very small, and the mean error for a single observation, since the observer was sometimes ahead and sometimes behind the true time, frequently, if not always, exceeds the average personal equation found.

In observing transits of the bead when it moved along a scale divided to tenths of a second, Hartmann noticed an interesting illusion. Sometimes when he knew beforehand the exact place where the star should be at the second stroke, he seemed to see it from 0.03 s. to 0.08 s. in advance of its true place. With particular effort to see exactly and extreme attention, the star seemed to stand still an instant at the place where it was when the stroke entered. At other times it seemed to advance steadily and was in motion in its right place at the stroke. This happened most frequently at the end of a series of observations or when the experimenter observed somewhat nonchalantly. He does not venture an explanation, but suggests that the differences may be caused by differences of attention; the star being most regarded in the first and third and the clock-beats in the second. Something similar he thinks possibly happens with the flashes of light, though he was not able with his apparatus to demonstrate it.

The conclusions to which this experimenter comes are:

First, that the absolute personal equation, when it amounts to a tenth of a second or more, is not necessarily grounded in the make-up of the eye or ear or in the mind; and that the "reception time" taken alone rarely rises above a few hundredths of a second. Second, that it finds its cause rather in unequal attention, surprise, defective memory of the series of light and sound impressions, wrong customs of observing, etc.—all of which may perhaps be helped by a knowedge of the error. Third, that it is not constant, but varies from day to day and from series to series, and even with the tenth of the second in which the phe-

nomenon happens to fall. A tendency to this last variation Hartmann thinks he is able to find also in the observations of Gauss and Goldschmidt on heliotrope and powder signals in the longitude determination before mentioned.

In 1863, F. Kaiser, of the Observatory of Leyden, published a method of getting the absolute personal equation quite different in the manner of measuring the time from those generally employed. It depends on the observation of the coincidence of the beats of two clocks beating at slightly different rates. An example will show how the thing is done. Suppose the observing clock beats forty-nine times to fifty beats of the standard clock, that is, beats every 1.02 s. At the occurrence of some phenomenon the pendulum of the observing clock is released, and its beats are counted till they reach a coincidence with those of the standard clock. If thirty-five beats are counted, and at the coincidence the standard clock reads 10 h. 42 m. 50 s., the true time of the phenomenon is gotten as follows: $35 \times 1.02 = 35.70$ s. as the time from the starting of the observing clock to the coincidence. This taken away from the time indicated by the standard clock leaves 10 h. 42 m. 14.30 s. as the true time of the phenomenon. To apply this to measuring the absolute personal equation for artificial transits, it is only necessary to have the pendulum held by an electro-magnet which shall release it on the breaking of the circuit, and to make that correspond with the bisection of the star. In Kaiser's apparatus the artificial transits were managed by placing at one end of a bar of wood a lamp, and before it a screen with a small hole in it; at the other end of the bar a lens which projected a fine image of the hole on another screen of oil paper where

the reticle was represented by a vertical black line. The bar was turned by clock-work, could be varied in speed, and was arranged to break the electric circuit at the instant of the passage of the star. When the absolute personal equation was to be taken, the observer made his estimate of the time of the transit by eye and ear, while an assistant counted the strokes up to a coincidence, and from that, as explained above, the real instant of the transit was found ; the difference of this from the observed time gave the personal equation. The apparatus could be varied to represent occultations and powder-signal flashes, and could be applied to measuring the personal equation in observing by the other method, though for that it was more complicated.

Experiments were made from 1851 to 1859. The following figures are from May of the latter year :

Observer.	Mean personal equation.	Prob. error.	Limits between which the personal equation varied.	
Gussew,	— 0.10	± 0.057	+ 0.07	—0.31
Brouwer,	+ 0.18	± 0.095	+ 0.33	—0.21
Kam,	+ 0.15	± 0.083	+ 0.29	—0.18
P. J. Kaiser,	+ 0.08	± 0.088	+ 0.29	—0.11

The accuracy of measurements of the personal equation by this method depends on the accuracy with which a coincidence of the clock-beats can be observed. Later experimenters have shown that such observations are themselves liable to a certain small personal error.

In 1862 he improved upon his former apparatus for artificial stars, and adapted the new one to chronographic recording. In the first months of 1867 the Kaisers (father and son), Kam, and Van Hennekeler

made observations to the number of several thousand with this apparatus, from which they concluded that for them the personal equation was small and, provided the circumstances of observation remained the same, constant; that it might be reduced by practice; that the motion of the star, alternately from right to left and left to right, made no difference, except for Van Hennekeler, who observed a little later when the motion was from right to left than when it was the reverse; and that when they observed motions up and down by the introduction of a prism, all were made later.

Another careful measurement was undertaken in 1862 by the Swiss astronomers Hirsch and Plantamour, in connection with the measuring of the difference in longitude between Geneva and Neufchatel. In May, 1861, they determined their relative personal equation by the observation of nine stars, using the chronographic method, and found P—H = — 0.082 s. In October of the same year, with somewhat unfavorable conditions, they found P—H = — 0.202 s. ± 0.020 s., from twenty-three stars with single values ranging from — 0.008 s. to — 0.413 s. In April of the next year, from the observation of twenty-four stars they found P—H = — 0.130 s. ± 0.008 s., with single values from — 0.068 s. to — 0.220 s.

The astronomers were unsatisfied with these results, and went on to the fixing of their absolute personal equations, using the transits of an artificial star, and taking the time with a Hipp chronoscope, measuring down to the thousandth of a second. Their artificial star, as seen in the telescope, was of the second or third magnitude. The disk (a hole in which made the star) was moved by a pendulum, and so fixed that

through its eastward excursion it kept an electric circuit closed and through its westward left it open. In observing, an assistant let the pendulum fall, and this moved the disk from west to east. As the image of the star crossed the wire, the observer gave a signal to a second assistant, who instantly started the chronoscope. As the star crossed the wire on its return, it automatically broke the electric circuit, and thus threw the pointers of the chronoscope into connection with its driving machinery. As soon as the observer saw the star behind the wire, he pressed his key, thus closing the circuit again and throwing the pointers out of connection. The assistant at the chronograph stopped its works, read off the fraction of a second that had elapsed between the opening and closing of the circuit, and the apparatus was ready for another experiment.

The prime fault of the chronoscope for measurements of this kind is that it fails to record anticipatory estimates, and, unfortunately, Plantamour anticipated. To bring his anticipations into calculation, it was assumed that all the estimates would arrange themselves symmetrically about the mean of the figures that would remain after a number of the latest observations equal to that of the anticipations had been temporarily excluded. The difference between the mean of the figures retained and the mean of the figures excluded is by hypothesis equal to the difference between the first mean and the mean of the anticipations, and they can thus be brought into the general average. Hirsch always observed too late, and so his figures needed no correction. Plantamour's corrected averages are also too late.

The following are the values found:

For Plantamour:

		s. s.			
Nov. 4,	2d series,	0.103 ± 0.013	from	45	observations.
"	3d "	0.128 ± 0.014	"	33	"
"	5th "	0.048 ± 0.009	"	41	"
" 5,	1st "	0.069 ± 0.007	"	54	"
"	4th "	0.037 ± 0.006	"	37	"
	Mean,	0.060 ± 0.016			

For Hirsch:

		s. s.			
Nov. 4,	1st series,	0.247 ± 0.043	from	6	observations.
"	4th "	0.178 ± 0.014	"	19	"
"	6th "	0.140 ± 0.007	"	41	"
" 5,	2d "	0.199 ± 0.009	"	22	"
"	3d "	0.169 ± 0.008	"	23	"
	Mean,	0.168 ± 0.013			

From which P—H = — 0.108 s. ± 0.021 s. Or, taking the same by consecutive series:

Nov. 4,	series 2 and 1,	P—H	= — 0.144
"	" 3 " 4,	"	= — 0.050
"	" 5 " 6,	"	= — 0.092
" 5,	" 1 " 2,	"	= — 0.131
"	" 4 " 3,	"	= — 0.133
		Mean,	— 0.114 ± 0.019

The united results of all their comparisons, taken by days and weighted according to their probable errors, gave P—H = — 0.122 s. ± 0.026 s., or grouped by series, — 0.123 s. ± 0.015 s., and these values were finally taken as certain. The mean value of accidental variations in the same series was found equal to 0.056 s.; the value of changes due to the disposition of the

observer ranged between 0.03 s. and 0.04 s. The changes from day to day seem about equal to those from year to year.

One of the most extended studies of the absolute personal equation was made in 1863 and 1864 by C. Wolf, of the Observatory of Paris. Like previous experimenters, he made use of artificial transits. The light of an oil lamp shining through a hole in a black screen made the star, which, by the interposition of lenses, appeared in the telescope as a very small point of light. The screen was moved evenly to and fro by a motor under control of the observer, and its rate could be changed by changing the driving weight. The reticle of the telescope used in the experiments was of five lines. The blunt end of a spring under the screen made contact with an adjustable strip of copper at every bisection of the star by a line of the reticle; or when the direction of its motion was reversed, broke contact at the same point. The making and breaking of the circuit caused the registry of the transit by an electro-magnet on a moving band of paper, where in like manner the clock-beats were also recorded. In some cases registry by an induction spark was substituted. The total length of the apparatus, as set up in Paris, was 4.25 meters. Forty transits, that is, the crossing of the reticle four times in each direction, constituted a full series. The offices of the different parts of the apparatus were so alternated during these as to exclude the errors of the instrument, and determinations by means of it are certain to 0.01 s.

Either the chronographic or the "eye and ear" method might have been studied, but Wolf considered that his personal equation by the chronographic

method would be too small to afford hope of finding its laws, and therefore he confined his attention to the older method. In the first three months of experimentation his personal equation declined from 0.30 s. to 0.11 s., and afterward, under like circumstances, remained constant at that point, which he regards as an evidence of the value of training on appropriate apparatus. Since the observation of the star with motion both from right to left and *vice versa* was essential to the elimination of instrumental errors, Wolf found it necessary to investigate the influence of the direction of motion on the personal equation. The introduction of a total reflecting prism before the eyepiece each time the star moved in one direction or the other, and the removing it when the motion was reversed, made the star appear to move always in the same direction, and gave the means of studying the effect of direction without altering the action of the instrument. He found from twenty-two series of observations, with the star moving at the equatorial rate and the normal power of the eye-piece, the following means for his absolute error:

Motion from left to right,	0.14
" " right to left,	0.10
Difference,	0.04

That is to say, the distance of the star from the line whenever the star was found to the right of it at the beat of the second seemed relatively too large to him. Tests with dots and lines on paper gave a similar illusion and thus fixed its origin in the structure of the eye.

Wolf studied as well the effect of the degree and manner of the illumination of the field, the brightness

of the star, and the position of the head of the observer, without finding that they influenced the size of the personal correction. The rate of the star made a difference. With variations which changed the time of crossing the five lines of the reticle from thirty-one to eighty-seven seconds, he found that his personal equation decreased with the rate, from 0.14 s. in the first case to 0.09 s. in the second; the star at the equatorial rate crossed in sixty seconds and his personal equation was then 0.11 s. With increase in the power of the ocular his personal equation diminished, but this he does not regard as evidence of a law, because under the higher power a line of the reticle appears to have a sensible breadth, and the place of the star may not always be estimated from exactly the same point of it. And besides, the diameter of the image of the artificial star is increased, which is not the case with actual stars.

The second part of Wolf's study deals with the cause of personal equation, and as this is to be considered elsewhere, only his general conclusions will be given here. He would make three kinds of personal equation: the first, very rare, consists chiefly in a miscounting of the seconds, as is charged against Bessel; second, a more common form and less in amount, caused by the difficulty of superposing the sensations, an error of imperfect training; and third, the true physiological personal equation, much smaller and more constant, which is a function of the sensibility of the eye alone. The "eye and ear" method is necessarily better than the chronographic, because the latter involves the same question of the sensibility of the eye, and besides that, of the time necessary to press the key. If it be true, as Dunkin and Pape have

shown, that the average error of a single observation is less by the chronographic method, it only argues that astronomers need more training.

Since the question with artificial-star measurements of the absolute personal equation is whether or not they really correspond to the conditions of actual observation, it is interesting to notice a suggestion made in 1867 by E. Kayser for the determination of the absolute personal equation from the transits of real stars. If a star is bisected by a wire of the reticle of an equatorial instrument, even if the driving clock have considerable errors, it will be able to preserve the bisection for a short time. It is only necessary, then, after making such a bisection, to stop the driving clock on a beat of the time clock and observe the transits of the star across the other wires, the true times of which can be accurately found from the known distances between them. Since the stopping of the clock on a second beat is only hitting one of a rhythmical series, the error need not be large. If the chronograph method is used, the thing is still simpler ; it is only necessary to make the stopping of the driving clock record itself on the chronograph. This gives a beginning from which the time of the transits, also recorded on the chronograph, can easily be read off and the absolute personal equations determined as before.

Since 1867 there have been a number of studies of personal equation, but most of them have been upon changes produced in its amount by varying conditions of observation. These it is proposed to bring in in their appropriate places (together with others passed by in the present narrative) in the next section, which is to deal with the variations of the personal equation.